other type be substituted for E′ it is easy to find its x, for its α is as area of cups, its f' is known, and assuming its x' and computing as before, we get similarly its Δx. He tried five different types and obtained very unexpected results, for he found that the x varied as some inverse function of the diameter of the cups and of the arms. He gives its values.

No. 1.	Original instrument	12″ cups	23·17 arms,	x=1·5880,	limit	2·812
,, 2.	Kew	9 ,,	24, ,,	1·5919,	,,	2·831
,, 3.	,, ...	9 ,,	12, ,,	1·7463,	,,	3·035
,, 4.	,, ...	9 ,,	8, ,,	2·1488,	,,	4·051
,, 5.	,, ...	4 ,,	26·75, ,,	1·8587,	,,	3·425
,, 6.	,, ...	4 ,,	10·67, ,,	2·5798,	,,	4·958

No. 6 is similar to No. 2, and it might be expected that their constants would be equal. The cause of these differences is partly the eddies caused by the cups being more powerful when the arms are short, but still more the presence of high powers of the arm and diameter occurring in the expressions of the mean pressures on the concave and convex surfaces of the hemispheres. In the present state of hydrodynamics we cannot assign these expressions, but we know enough to see that such powers may be present.

As each type of anemometer has its own constants, the author would suggest to meteorologists the propriety of confining themselves to one or two forms. For fixed instruments he considers the Kew one as good as any, and would wish to see it generally adopted. For portable ones he has no experience except with Casella's 3″ cups 6″ arms, which he found very convenient; he has not however determined its constants. Some selection of the sort seems necessary if it is wished to have an uniform system of wind-measures.

XIII. "Note on the Bearing on the Atomic Weight of Aluminium of the Fact that this Metal occludes Hydrogen." By J. W. Mallet, F.R.S. Received June 14, 1880.

In a recent communication to the Académie des Sciences,* it has been pointed out by Dumas that metallic aluminium and magnesium, as well as silver, may contain sensible quantities of occluded gas, that obtained from aluminium being almost pure hydrogen.

In a paper on the atomic weight of aluminium read before the Royal Society on the 22nd of April last I gave, among other data, the results of several experiments on the quantity of hydrogen liberated by a known weight of aluminium from a strong solution of sodium hydrate.

* "Comptes Rendus," 3 Mai, 1880, p. 1027.

In examining beforehand the purity of the specially prepared metal used, warned by Dumas' previous results as to gaseous occlusion by silver, of which due account was taken in the paper, I did not neglect to test in like manner the aluminium, but obtained an entirely negative result. As, however, I heated the metal in a Sprengel vacuum only to the highest temperature which a hard Bohemian glass tube would bear, while M. Dumas says that gas is only given off suddenly "*vers le rouge blanc*," I have since seeing his paper repeated the experiment in a porcelain tube, and at this higher temperature, obtained by means of an excellent gas furnace, with a small remnant of the same material used in the atomic weight determinations.

4·783 grms. of the metal thus treated gave a small bubble of gas, which measured but ·75 cub. centim. at 24° C. and 752 millims. pressure. Transferred to a miniature eudiometer it was exploded with oxygen, and seemed to be pure hydrogen. This is but little more than one-third the quantity of gas obtained by Dumas, but his experiments and my own with silver prove that the amount occluded varies with the conditions under which the metal has been fused, and two circumstances in the preparation of my aluminium probably tended to reduce the proportion of hydrogen taken up—namely, that, fearing possible contamination of the metal by its alloying itself with sodium at the moment of reduction from the bromide, I used a considerable excess of the latter, and that, not only were the ingots of sodium wiped free of naphtha with a cloth, but the outside surface was pared off with a knife, and only quite clear and solid pieces of the alkaline metal were used for the reduction.

The above volume of hydrogen only represents a weight of 0·000061 grm.; the corresponding weight from the largest amount of aluminium used in the atomic weight experiments of Series 3, A (in which the gaseous hydrogen evolved was *measured*), would be but 0·000010 grm., and the corresponding weight from the largest amount of aluminium used in Series 3, B (in which the hydrogen evolved was burned and weighed as water), would be 0·000067 grm.—both quantities too small to be determined by the balance used.

Even if it were possible to correct for this occluded hydrogen the weight of metallic aluminium taken, the greatest error in the volume of hydrogen obtained in Series 3, A, would only represent 0·12 cub. centim., which would be barely measurable in the flask used, and would affect the atomic weight deduced for aluminium to an extent less than one-tenth of the probable error of the mean result. And, in Series 3, B, the greatest error in the weight of the water produced by combustion of the hydrogen would be 0·0006 grm., measurable by the balance, but affecting the result by less than three-fourths of the probable error of the mean.

Hence it appears that, although there is undoubtedly a constant

error due to the cause pointed out by the great French chemist, its influence is in the present case inappreciable by existing means of measurement, and no correction of the numbers obtained for the atomic weight in question can be applied which shall have any real meaning.

The other series (1 and 2) of experiments made to determine this atomic weight do not involve the question of gaseous occlusion, at any rate in the same form, as in them the metal itself was not used, but certain of its compounds only.

XIV. On the Spectrum of the Flame of Hydrogen." By William Huggins, D.C.L., LL.D., F.R.S. Received June 16, 1880.

Messrs. Liveing and Dewar state, in a paper read before the Royal Society on June 10 (*ante* p. 494), that they have obtained a photograph of the ultra-violet part of the spectrum of coal gas burning in oxygen, and in a note dated June 8 they add that they have reason to believe that this remarkable spectrum is not due to any carbon compound but to water.

Under these circumstances I think that it is desirable that I should give an account of some experiments which I made on this subject some months since without waiting until the investigation is more complete.

On December 27, 1879, I took a photograph of the flame of hydrogen burning in air. As is well known, the flame of hydrogen possesses but little luminosity, and shows no lines or bands in the visible part of the spectrum, except that due to sodium as an impurity.

Professor Stokes, in his paper "On the Change of Refrangibility of Light,"* had stated that "the flame of hydrogen produces a very strong effect. The invisible rays in which it so much abounds, taken as a whole, appear to be even more refrangible than those which come from the flame of a spirit lamp." I was not, however, prepared for the strong group of lines in the ultra-violet which, after an exposure of one minute and a half, came out upon the plate.

Two or three weeks later, about the middle of January, 1880, I showed this spectrum to Professor Stokes, and we considered it probable that this remarkable group was the spectrum of water. Professor Stokes permits me to mention that, in a letter addressed to me on January 30, he speaks of "this novel and interesting result," and makes some suggestions as to the disputed question of the carbon spectrum.

* "Phil. Trans.," 1852, p. 539.